Can constructive empiricism give an account of novel predictions of unexpected phenomena?

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Abstract

In this paper it will be addressed the possibility of a constructive empiricist account of novel predictions of unexpected phenomena by new physical theories. In particular it will be considered two historically important cases, the prediction of the bending of light in Einstein's theory of gravity, and the prediction of antimatter in quantum electrodynamics. It will be show that, as it stands, constructive empiricism does not give an account of novel predictions. In this way constructive empiricism does not seem to give an adequate account of physical theories.

1. Introduction

There is a well-known claim that we can only make sense of the predictive success of science through realism. I will not consider this subject here. My take will be on what is usually considered the reverse side of this claim, which entails that anti-realist views on science are not able to make sense of science's predictive success. I will consider only the case of van Fraassen's constructive empiricism.

In part 1, I will set the stage for the discussion by addressing A. Musgrave and B. C. van Fraassen's views on the subject. In part 2, I will present two 'case-studies': the prediction of the bending of light in Einstein's theory of gravity, and the prediction of anti-electrons in quantum electrodynamics. I will defend the view that we can separate novel predictions in two sets: general and specific novel predictions. This might open the door for an underdetermination argument regarding general novel predictions; to address the possible problem for realism of this situation it is necessary an account of intertheoretical relations. In part 3 I will address the question: can constructive empiricism give an account of novel predictions of unexpected phenomena? by taking into account the results of part 2, and this implies putting intertheoretical relations in the central stage of the discussion. I will defend the view that constructive empiricism cannot give an answer to the question even in a limited relative way, by relating general novel predictions given by different theoretical frameworks to a particular benchmark theory.

2. On Musgrave and van Fraassen's views

Regarding the predictive success of physical theories, Musgrave stresses the need to distinguish between a theory's capacity to accommodate known phenomena from the prediction of unknown phenomena (Musgrave 1985, 210-11). Thus, Babylonian astronomy was capable of predicting lunar eclipses with an high accuracy, as it did Ptolemaic astronomy (Musgrave 1988, 231). Nowadays we do not consider these as examples of scientific theories, they are seen more as mathematical models that were able to accommodate observed regularities.

A completely different situation occurs, according to Musgrave, when a new theory predicts new (yet not observed an unexpected) phenomena. Musgrave thinks that

realism can give a good account of this, and thinks that anti-realist accounts, in particular constructive empiricism, cannot. In Musgrave's view, when van Fraassen addresses this subject in his *The Scientific Image*, he only provides a Darwinian metaphor for making sense of maintaining empirically adequate theories: only successful theories survive (van Fraassen 1980, 39-40; Musgrave 1985, 210). Musgrave criticizes this argument on account that 'to say that only successful theories are allowed to survive is not to explain why any particular theory is successful' (Musgrave 1985, 210). That is, according to Musgrave, van Fraassen does not give an account of the prediction of new phenomena by a new theory in his constructive empiricism.[1, 2]

In 2006 van Fraassen provided a further remark related to the so-called no-miracles argument (while not addressing explicitly the 'problem' of novel predictions): 'the success of science is *not a miracle*, because in any theoretical change both the past empirical success retained and new empirical successes *were needed as credentials* for acceptance' (van Fraassen 2006, 298-9)

Van Fraassen made this remark in the context of rebuffing structural realism's claim regarding a possible structural continuity between an old theory and a new superseding theory.[3] To van Fraassen there is no need for a putative structural continuity to cover the continuity in empirical adequacy, it is only necessary that the new theory implies approximately 'the same predictions for the circumstances in which the older theories were confirmed and found adequately applicable' (van Fraassen 2006, 298), i.e., the new theory must duplicate the empirical success of the past theory (or equivalently it must retain/accumulate the past empirical knowledge or empirical structure; see van Fraassen 2006, 298-301). I doubt that Musgrave would accept this 'newer' remark as providing a constructive empiricist account of how new theories are able to predict novel unexpected phenomena. Van Fraassen's comments give no indication to a possible answer. In fact they can be seen as simply referring to the methodology adopted, according to some, in scientific research, i.e. the matching and superseding of the empirical adequacy of an older theory by a new theory is a *sine qua non* condition for the acceptance of the new theory.

In Musgrave's view there are still some loss-ends in framing beyond an intuitive level a relevant notion of new prediction: 'Difficulties remain, of course, not least that of making precise the intuitive distinction between known effects and novel predictions' (Musgrave 1985, 211). Van Fraassen develops a related point by picking up W. Whewell's notion of independence: the prediction of new unexpected phenomena can be

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[1] Let us recall that in van Fraassen's view only empirical adequacy (i.e. saving the phenomena) counts as criteria for acceptance of a theory (see, e.g., van Fraassen 1980, 12).

[2] Musgrave and van Fraassen's arguments are made in the context of the so-called no-miracles argument. This is taken to be a positive general argument that says that only realism 'doesn't make the success of science a miracle' (Putnam 1975, 73). Arguments against and in favor of this argument have been made. According, for example, to C. Howson (2000, 52-4), P. Lipton (2005, 1267), and P.D. Magnus and C. Callender (2004, 323-29) the argument rests on a fallacy. However this view is made regarding only a probabilistic version of the argument. Another way of presenting the argument is as an inference to the best explanation. Here too we face a possible problem, now of circularity (Magnus and Callender 2004, 330-31; Newman 2010, 112). Again maybe this only shows that the argument should not be presented as an inference to the best explanation. A more sympathetic view regarding the no-miracles argument was made by G. Schurz defending a restricted form of the argument based on a proposed structural correspondence between a superseded theory and a new theory (Schurz 2009). I will not address here directly the no-miracles argument, only the question about constructive empiricism eventual account of novel predictions.

[3] It seems that van Fraassen sees the relation between the 'new' and 'old' theory in a way that has similarities with some realist's views in terms of theory reduction: the 'new' theory is completely superseding the 'old' theory (even if just in terms of empirical structure).

taken to be independent support for the new theory. In van Fraassen's view the notion of independence must be related to the existing theories and experimental knowledge at the time the new theory was being developed; accordingly, 'the old charge of arbitrariness lurks nearby, of course, for this is to some extent a matter of historical accident' (van Fraassen 1985, 267). This might create a doubt about to what point (or in what sense) are 'new phenomena' really new or simply dependent of an historical cultural context that sees them that way and possibly, because of this, philosophically the prediction of new unexpected phenomena might not be so important after all?

In my view the situation is not one of relativism or historical contingency as these previous views might led us to think; in terms of intertheoretical relations it is possible to define with clarity what are new phenomena, which we can regard as constituting independent support. Musgrave himself called the attention, in a later work, to the possibility of making a clear distinction between known predictions and novel predictions: 'a predicted fact is a novel fact for a theory if it was not used to construct that theory' (Musgrave 1988, 232). I do not think this is the best option for framing a meaningful notion of novel prediction. In fact, is it not possible that there are predicted facts (of a new and old theories), not used in the construction of the new theory, that are not novel predictions? We would need to make an exhaustive list of all 'predicted facts' to disentangle this matter.

I prefer a more conservative 'definition' of novel predictions based on comparing 'new' and 'old' theories (i.e. on intertheoretical relations), even if this might not be the 'minimal' possible definition; also this 'definition' is to be applied case by case.[4] In this way, I take the prediction of the bending of light in Einstein's gravitation theory to be a novel prediction (not known and not expected according to Newton's gravitation theory; the 'old' theory in this case); another example is the novel prediction of anti-electrons made in quantum electrodynamics (unexpected according to relativity theory and classical electrodynamics; the 'old' theories in this case). Now, constructive empiricism seems to be facing a problem. Are not these novel predictions taken to be independent support to the corresponding physical theory? I think they are. They are not part of the phenomena saved by the older theory, neither they are part of the phenomena not saved by the old theory and expected to be saved by the new theory, i.e. they are an (eventual) independent support for the new theory. The theory is predicting new regularities in nature. It will be empirically adequate only if observation and/or experimentation agree with the theory's novel predictions. Can constructive empiricism, being ontologically agnostic and taking physics to be an enterprise of construction and not of discovery as realists see it (see, e.g., van Fraassen 1980, 5), make sense of the anticipation of regularities of nature?

3. The distinction between general and specific novel predictions

3.1. Two case-studies: the bending of light and antimatter

Two well-known novel predictions in 20th century physics are the bending of light due to gravity and the ‘existence’ of antimatter.

According to Newtonian gravitation there is no bending of light due to gravity and there is no reason to expect such phenomenon. It is a novel prediction of Einstein’s

[4] This view agrees with van Fraassen's comment that we must take into account existing theories, but there is no historical contingency here. It is not important if a particular novel prediction is an historical accident; what matters is that there are novel predictions, and this is not an historical contingency.

theory of gravity which as been confirmed in 1919 (see, e.g., d'Inverno 1992, 199-201). Looking closely into the derivation of this prediction within Einstein's theory of gravity we see that it is already derived using the so-called parametrized post-Newtonian (PPN) formalism, which contains the post-Newtonian approximation of different metric theories of gravity as a special case:[5] by choosing a particular set of parameters the PPN formalism gives the post-Newtonian limit for different theories of gravity (see, e.g., Misner, Thorne, and Wheeler 1973, 1069). In fact the bending of light is a very general prediction that can even be derived without a dynamical theory of gravity; by considering a relativistic kinematical principle (the equivalence principle) that relates observed phenomena in a constant gravitational field to observed phenomena in a constantly accelerated reference frame, it is already possible to predict the bending of light (even if this approach only predicts half of the deflection predicted by Einstein's theory of gravity; see, e.g., Callahan 2000, 191-195). The situation is then the following:

1) The bending of light can be predicted in the post-Newtonian approximation, not being necessary to take fully into account Einstein's field equations,

2) It can be derived by other rival theories (using an equivalent PPN approximation);

3) It can be derived by a general principle (the equivalence principle) defined within the 1905 theory of relativity.

We see that there is an 'atmosphere of generality' in all this; apparently it is not a prediction that comes out only of Einstein's field equations. I see the possibility for two excluding options here:[6]

1) Maintain the view that the bending of light is a novel prediction of Einstein's theory of gravity.

2) Do not regard the bending of light as a new prediction of Einstein's theory of gravity, but as a prediction already made within the *different* relativity theory framework.

A realist would possibly choose option 1, since it would open the door to the possibility of showing that only Einstein's theory of gravity gives a consistent derivation of the bending of light and that the equivalence principle derivation could in fact be seen as an approximation or better a work in progress that only makes sense from the perspective of the later developed theory. With this option the realist would possibly only have to confront the eventuality of the underdetermination of Einstein's theory of gravity (see, e.g., Lyre and Eynck 2003), which he might intent to undermine by defending a structural realism that might not be affect in its ontological commitments by the possible underdetermination of the theory.[7] The realist would certainly reject option 2

[5] The post-Newtonian approximation consists in the case of a weak gravitational field and slow motions that enable to describe gravitational phenomena in terms of second-order (post-Newtonian) corrections to the (first-order) Newtonian treatment.

[6] I will not consider the case of alternative theories of gravity since, in the present time, there are not convincing rivals to Einstein's theory of gravity (see, e.g., Will 2006).

[7] The possible underdetermination of physical theories is usually considered an important argument against realism (see, e.g., Ladyman 1998); there are authors that consider that structural realism avoids any possible underdetermination problem (see, e.g., French and Ladyman 2003; Cao 2003; Worrall 2009; Bain 2009).

because it would open the door for a strong form of underdetermination in which *different* theoretical frameworks give the same novel predictions.

An anti-realist would reject option 1 or at least consider that even for structural realism there still would be a serious case of underdetermination (see, e.g., Lyre 2009). The best option for the anti-realist seems to be 2 since, as mentioned, it opens the door for a strong form of novel prediction underdetermination between non-reducible theoretical frameworks. This could be presented as a negative argument against the realists claim that considers novel predictions as evidence for realism, but by itself is not a positive argument showing how, for example, constructive empiricism can make sense of novel predictions.

Both the realist and the anti-realist would have to provide a conclusive argument to show the theoretical continuity or discontinuity between the theory of relativity and Einstein's theory of gravity (i.e. they must address questions regarding intertheoretical relations). There is an alternative to having to choose immediately between options 1 and 2 that enables a correct characterization of the situation by stressing the *de facto* situation that there is a different degree of application of theories in the prediction of different novel phenomena.

Instead of thinking in terms of novel predictions of one or the other theory one can think in terms of general novel predictions and specific novel predictions. In this way the bending of light can be seen as a general prediction of Einstein's theory of gravity which can also be made from the theory of relativity and a kinematical principle, and, for example, the prediction of black holes can be seen as a specific prediction of Einstein's theory of gravity since it arises only by consider an exact solution of Einstein's field equations (see, e.g., Ludvigsen 1999, 134-155). It is the general novel predictions that bring the possibility of a strong underdetermination of novel predictions.

To show that this way of addressing novel predictions in terms of general or specific predictions can be applied more generally let us consider another example where the differentiation makes sense.

As it is well-known, P. A. M. Dirac, using his relativistic wave equation, predicted the 'existence' of antimatter, which was an unexpected result according to classical theories and was confirmed in the early thirties (see, e.g., Schweber 1994, 66-9). Again, like in the previous case, this was an unexpected phenomenon according to previous accepted theories. However, like in the case of the bending of light, we are facing here an 'atmosphere of generality' since this is not a specific prediction of quantum electrodynamics. In reality already the so-called Klein-Gordon equation predicted antimatter.[8] Dirac himself noticed that the prediction of antimatter followed from any relativistic wave equation (for charged particles), due to the fact that the relativistic expression for the energy of a particle has the form $E^2 = m^2c^4 + c^2p^2$ (Dirac 1930, 360). In this way, antimatter is not a novel prediction specific to quantum electrodynamics.

From a realist perspective this situation might seem to be unproblematic. According to S. Weinberg, local quantum field theory, with its prediction of negative and positive charged quanta – and quantum electrodynamics can be seen as a particular instantiation of this –, is the only theoretical approach that satisfies three assumptions: the principle

[8] This equation was first developed by E. Schrödinger when looking for a wave equation to describe quantum phenomena. Using this wave equation Schrödinger determined the energy levels for the hydrogen atom, and arrived at a result that was not in agreement with Sommerfeld's result for the hydrogen spectrum (obtained within the so-called old quantum theory), and so he drooped it (Kragh 1981, 32-7). It was only in 1935 that the equation reappeared when it was shown that it could give a physical description of Mesons (Kragh 1984, 1031).

of Lorentz invariance (i.e. the theory of relativity), the quantum formalism, and the so-called cluster decomposition principle (Weinberg 1999). J. Bain uses this result, even if it is not a strict theorem, to defend the impossibility of underdetermination of local quantum field theory (Bain, 1999).

From an anti-realist perspective one might try to overturn the realist position by stressing the fact that more than a result that follows imperatively from a (quantum field) *theory*, it is a prediction that is made previous to quantum field *theory* proper (as the prediction of the bending of light can be made previous to Einstein's theory of gravity) by a *quantization* of any (*relativistic*) charged classical field like for example the complex scalar field (see, e.g., Bogoliubov and Shirkov 1959, 35-8).

Again, like in the previous case, each option would need a clear account of intertheoretical relations, in this case between the theory of relativity, classical electrodynamics, quantum theory, and quantum electrodynamics.

A simpler option, at this point, is to frame the discussion in terms of general or specific novel predictions (which does not involve engaging into details of intertheoretical relations). Antimatter can be seen as a general prediction made for any (charged) quantum field, while, for example, the light-light scattering can be seen as a specific prediction of quantum electrodynamics that is not expected according to classical electrodynamics (Schweber 1962, 558-9).

Summing up the results so far: the two novel predictions (the bending of light and antimatter) are both general predictions not specific to Einstein's theory of gravity and quantum electrodynamics. They can arise in one case from the theory of relativity and a kinematical principle (previous to any dynamical theory of gravity), and in the other case from any quantization of a relativistic wave equation for charged matter. By framing the discussion in terms of general novel predictions we have a new perspective from which to discuss realist and anti-realist positions regarding novel predictions, in particular by pointing to the possibility of a strong underdetermination of the general predictions. However one should not forget that this is a negative argument against realism not a positive argument that makes sense of novel predictions from an anti-realist position.

3.2. Kirchhoff's prediction of light diffraction as a general prediction

J. Saatsi and P. Vickers presented a case-study that in their view might undermine certain realist positions (Saatsi and Vickers 2011), based on Kirchhoff's derivation of a diffraction equation within an ether paradigm of propagation of (scalar) light waves for the case of light incident on a screen with a small aperture. This equation predicts – in similar lines to Fresnel's 'novel' prediction (when compared with Newton's corpuscular theory of light) of a bright spot in the center of the shadow of a small circular disk – a diffraction pattern in the intensity of light on the other side of the screen. Saatsi and Vickers call the attention to the fact that Kirchhoff's derivation relied on physically wrong assumptions (from the point of view of Maxwell's theory), which are even mathematically inconsistent:

> it turns out that Kirchhoff's derivation turns on crucial assumptions regarding the *amplitude* of light waves that (i) *differ considerably from the actual situation* (as described by Maxwell's equations, for example) in various respects, and (ii) as a matter of fact are *inconsistent* (Saatsi and Vickers 2011, 30);

> the 'success-fuelling' assumptions are radical wrong (Saatsi and Vickers 2011, 31).

From this, in my view wrong, assessment of the situation Saatsi and Vickers go on to conclude that realists have a problem since the predictive success of the theory seems to depend on wrong assumptions (Saatsi and Vickers 2011, 42).[9]

As Saatsi and Vickers called the attention to, Kirchhoff's equation can be derived from a set of consistent assumptions within the theoretical framework where Kirchhoff develops his approach, that of a scalar wave theory based on the idea of an ether taken to be the bearer of light waves (Saatsi and Vickers 2011, 39). They go on to consider these two approaches, Kirchhoff's original one and the newer consistent approach, as different theories (Saatsi and Vickers 2011, 39), while they call the attention that these are not fundamental theories (Saatsi and Vickers 2011, 36). I would instead prefer to call them different models developed within the same theoretical framework.[10] By calling the models theories, Saatsi and Vickers can claim that we are facing a case of underdetermination.[11] Even conceding to Saatsi and Vickers that the two identical equations developed in the ether paradigm can be considered as two diffraction *theories*, it is still the case that, contrary to Saatsi and Vickers' claim, Kirchhoff's equation does not depend *crucially* on his wrong assumptions:

> all of the possible ways in which Kirchhoff's assumption (A1) differs from the truth (according to Maxwell's equations), it just so happens that the difference has negligible effect (Saatsi and Vickers 2011, 41).

That is, mathematically Kirchhoff's wrong assumptions do no affect the final form of the equation. Accordingly, Saatsi and Vickers consider that 'due to the nature of diffraction there is a many-to-one mapping, so to speak, from amplitude-distribution-at-the-aperture to diffraction patterns' (Saatsi and Vickers 2011, 43).

I think it is possible to relate this characterization of the situation regarding Kirchhoff's 'theory' to the one presented in section 2.1 regarding the bending of light and the prediction of antimatter. From my perspective what Saatsi and Vickers call limited or local underdetermination, in the context of regarding as a theory Kirchhoff's derivation of an equation for the particular case of light incident on a screen with an aperture, can be rephrased by saying that Kirchhoff's prediction of a diffraction pattern is a general prediction made within an ether theoretical framework, and that (eventually) it can be made a case for a strong undetermination of novel predictions due to this (since that, even if historically not exact, we can see Kirchhoff's result as part of the novel predictions made within the wave theoretical description of light in contrast to the corpuscular theory of light). In my view this is where the (possible) cash-value for anti-

[9] It is important to notice that Saatsi and Vickers are presenting a negative argument that might undermine (to some extent) realist positions. Nowhere they present a positive argument showing how an anti-realist approach might account for Kirchhoff's 'novel' prediction.

[10] Looking into the details of both derivations, the main difference is that Kirchhoff makes a very idealized (and mathematically inconsistent) assumption regarding the amplitude of the light waves at the aperture. He considers that the screen does not perturb the light waves passing through the aperture, which is wrong from the point of view of Maxwell's theory (Saatsi and Vickers 2011, 34-6). A consistent formulation of Kirchhoff's approach is possible by making a more 'correct' assumption regarding the amplitude of the light waves at the aperture by taking into account the scattering of light off the edges of the aperture (Saatsi and Vickers 2011, 39).

[11] Saatsi refers to it as 'local underdetermination' (Saatsi, 2010, 4) or 'limited underdetermination' (Saatsi and Vickers 2011, 43). I will defend below that independently of regarding Kirchhoff's equation as a theory or not we can relate Kirchhoff's prediction to what I called general predictions, and, in this way, when adopting an anti-realist position, it might be possible (with the appropriate argumentation in terms of intertheoretical relations that Saatsi does not provide) to defend the possibility of what I called strong novel prediction underdetermination.

realists is, not in the fact that Kirchhoff deduces his equation in part from wrong assumptions.

4. The problems in van Fraassen's account of empirical success and intertheoretical relations

Framing the discussion in terms of general and specific predictions enables to separate novel predictions in two groups in which one of them can be problematic for realists. It does not offer any positive account (for either side) of novel predictions, but it shows that also realists might have problems facing novel predictions: these might be too general to be appropriately accommodated even by structural realism. To settle the matter (regarding the negative underdetermination arguments) between realists and anti-realists it is necessary, as we have seen, to go into details of intertheoretical relations.

According to van Fraassen, in constructive empiricism a new successful theory is only related to the old theory in the most minimal way of duplicating the empirical structure of the old theory, i.e. it must be empirically adequate where the old theory was. This means that according to van Fraassen there is no requirement of reducibility between the theories (van Fraassen 2006, 299). In his words, 'the relation is much looser, but nevertheless very demanding. The difference is that it grants conceptual autonomy to the new theory, which is allowed to re-describe nature entirely in its own terms' (van Fraassen 2006, 299). If this is the case then anti-realists have an argument to claim the existence of a strong novel prediction underdetermination, since it would not be possible to make sense of a novel prediction made within different theoretical frameworks by relating the different derivations to the derivation made in a particular benchmark theory. It does not give an anti-realist account of novel prediction but it would show that realists are not better off.

I have two reasons to doubt the strength of van Fraassen's position. Let us consider the first one. As already mentioned, van Fraassen thinks one can frame empirical success of past theories simply by calling the attention to the fact that they give approximately the same predictions as the new theories (van Fraassen 2006, 298); also the empirical adequacy is related, according to van Fraassen, only to 'knowledge about the observable phenomena' (van Fraassen 2006, 298); and it does not imply any structural knowledge about reality (van Fraassen, 2006, 299-301). In fact van Fraassen goes to the point of saying that the empirical success of theories, be it old or new (since an old theory was once a new one making novel predictions), 'consisted in their success of fitting the data, the deliverances of experimental and observational experience' (van Fraassen 2006, 303). This is simply not the case because we know that, contrary for example to Babylonian astronomy, physical theories actually predict novel phenomena they were not intended to save. Here is then the first problem for constructive empiricism:

Problem 1: How to understand a general novel prediction from the perspective of a possible adopted benchmark theory (since according to van Fraassen there is no continuity at a theoretical level)? For example, how to understand the prediction of the bending of light within the theory of relativity from the perspective of Einstein’s theory of gravity?

This is a 'little' version of our 'big question': how to understand novel predictions. The importance of the ‘little question' is that van Fraassen is unable to answer it. That is, he

cannot even give a relative answer regarding a novel prediction made within a particular theoretical approach in terms of the relation that this might have to a 'fundamental' theory. This is a strong evidence for the impossibility of giving an answer in absolute terms (for example by addressing the novelty of the bending of light within Einstein's theory of gravity). That it, at the same time that van Fraassen's views about intertheoretical relations bring the possibility of strong novel prediction underdetermination it also makes impossible to van Fraassen to give a positive relative answer about novel predictions in terms of the relation of general derivations to a particular benchmark derivation. In this way, it seems that van Fraassen's constructive empiricism does not offer a convincing characterization of physical theories since, for example, it is not able to distinguish between Babylonian astronomy (without any novel prediction beyond the data it was intended to save) and Newton's theory of gravity (that for example 'predicted' a yet-unknown planet, Neptune; see, e.g., Hirst 1946).

The second problem, which will bear on the first, is van Fraassen's account of intertheoretical relations in terms of a loose relation between theories without any continuity at the theoretical level (van Fraassen 2006, 300). This seems to be in contradiction to the *de facto* situation we have in physics. Considering only the case-studies presented in this work, in the case of quantum electrodynamics, it is developed (in part) by the application of quantization rules (from quantum theory) to the electromagnetic field as described by classical electrodynamics; also Dirac's equation is developed in accordance with the theory of relativity (see, e.g., Dirac 1958, 255). It is difficult to see a theoretical discontinuity between quantum electrodynamics, (non-relativistic) quantum theory, classical electrodynamics, and the theory of relativity (which is relevant to understand the relation of quantum electrodynamics with other quantum field theories; see, e.g., Bogoliubov and Shirkov 1959). Regarding Einstein's theory of gravity again it is difficult to see the theoretical discontinuity proposed by van Fraassen. Even if there is disagreement regarding the role of the equivalence principle in Einstein's theory of gravity (see, e.g., Synge 1964; Friedman 1983), it is clearly the case that there is a close relation between Einstein's theory of gravity, the theory of relativity, and Newton's theory of gravity: in the case of a weak gravity the space-time is nearly flat. This means that we can consider gravity as a field on a Minkowski space-time where physics is described according to the theory of relativity; also in the case that velocities are small the mathematical expressions of Newtonian gravity are recovered (see, e.g., Wald 1984, 74-8). The second problem for constructive empiricism is then:

Problem 2: constructive empiricism does not seem to offer a convincing characterization of the relation between theories; for example, it overlooks the close relation that exists between Einstein's theory of gravity, the theory of relativity, and Newton's theory of gravity.

I think we can have a better understanding of the intertheoretical relations we really have in actual physical theories by considering theories and their relations in terms of a modular structure (Darrigol 2008). According to Darrigol,

> Any advanced theory of physics contains modules defined as essential components that are themselves theories with different domains of application. Different kinds of modules can be distinguished according to the way in which they fit in the symbolic and interpretive apparatus of a theory ... Modularity conveys some global unity to physics through the sharing of modules by diverse theories. (Darrigol 2008, 195)

Darrigol gives a characterization of physical theories in terms of what he refers to as: (1) the symbolic universe, (2) theoretical laws, (3) interpretive schemes, (4) methods of approximation; (1) and (2) correspond roughly to the conceptual-mathematical formalism of the theory. The interpretive schemes connect the formalism to idealized experiments, i.e. they give the meaning to the theoretical structure in terms of experiments. The approximations are made in the mathematical procedures that enable in practice the interpretive schemes to provide results to compare with concrete experiments.

According to Darrigol the modular structure is particularly important (and visible) in the symbolic universe and in the interpretive schemes of the theories. This modular approach provides a way to address intertheoretical relations that is less strict than the one defended by structural realism but not as loose as the one promoted by van Fraassen's anti-realism. In particular, Darrigol calls the attention to the idea that to compare two theories it is necessary at least that they share some schematic modules (i.e. modules pertaining to the interpretive scheme of the theories). One example is

> that of the experiment that Augustin Fresnel performed at the Paris Academy of Sciences to answer an objection to his diffraction theory. Siméon Denis Poisson had noted that according to Fresnel's wave theory there should be a bright point of light in the middle of the shadow cast by a disc. The corpuscular theory, even in a version including deflections of the rays by the rims of the disc, could not possibly yield this bright point, which Poisson judged absurd. Yet the experiment confirmed the prediction of the wave theory. The experimental setup only involved geometrical and primitive photometric modules that both theories shared. (Darrigol 2008, 208)

Another case is that of the relation between Einstein's theory of gravity and Newton's theory of gravity; while it is the case, according to Darrigol, that they do not share basic modules of the symbolic universe, that of Euclidean geometry and mechanics, it is still the case that they, at least, share some schematic modules:

> In rare cases, the two compared theories do not share basic defining modules such as Euclidian geometry or mechanics. This happens for instance when the predictions of classical and relativistic electron dynamics are compared, or when the predictions of Newton's theory of gravitation are compared to those of general relativity. It would seem that in such cases the shared interpretive schemes could only involve pre-spatial and pre-mechanical observations about the coincidence of two small material objects or the emission and reception of light flashes. This very limited conception of interpretive schemes may in principle allow the comparison of the two theories, for it permits an idealized coordination between theory and simple concrete procedures. In practice, however, physicists never rely on such a tabula rasa devoid of Euclidian theory, Newtonian mechanics, and other pre-relativistic theories. Comparative schemes involve approximate, local use of these older theories in a complex manner that would deserve systematic study. At any rate, the astronomical tests of general relativity all involve earth-based or satellite-based instruments whose functioning requires earlier accepted geometry and optics, even though the implied spacetime relations are essentially non-Euclidian and non-Minkowskian at large scale. (Darrigol 2008, 209)

It is important to notice that module sharing does not have to be at the level of the formalism of the theory and even in this case it does not have to imply the existence of structural continuity in the same sense of structural realism. According to Darrigol,

> To sum up, the modular structure of theories implies their interrelatedness without yet implying reduction to a single fundamental theory. At the same time, this structure implies some fundamental heterogeneity of any non-trivial theory. (Darrigol 2008, 220)

In the modular approach, theories do not form a disconnect cluster whose only link is a partial superposition of empirical structure. This undermines the possibility for the

negative argument of a strong prediction underdetermination between disconnected theoretical approaches. More than that, the modular view of intertheoretical relations provides a scheme to answer the 'little question' about novel predictions: we can from the perspective of a benchmark theory answer in a relative way the question of how other theoretical approaches give novel predictions, since at one level or another they share modules that provide the link between the different theoretical approaches that give the same novel predictions. This by itself does not imply that we have a positive answer to our 'big question': how to make sense of novel predictions; however contrary to van Fraassen's anti-realism, when adopting a modular view of physical theories it is open the possibility of developing an approach that might enable to make sense of novel prediction without falling pray of the criticism of structural realism (which according to the modular view does not give an appropriate description of intertheoretical relations). Another positive aspect of this approach is that when considering intertheoretical relations first, one avoids noisy interference from our preferred philosophical stance, be it realist or antirealist. The idea is to give first an, as exact as possible, characterization of the situation before developing the philosophical account. After all when endorsing a modular account of intertheoretical relations one is not imposing a particular philosophical view and this approach is possibly compatible with moderate forms of both realism and anti-realism.

5. Conclusion

According to the view developed in this paper, van Fraassen's constructive empiricism does not give an account of novel predictions, and there are good reasons to doubt that it might eventually come up with an account. As we have seen van Fraassen does not really consider the problem of novel predictions. This might be due to his confusing view (van Fraassen 1985, 267) mixing the possible historical contingency of particular novel predictions with the fact that there are novel predictions, i.e. that the physical theories 'saving the phenomena' goes beyond the phenomena they were originally intended to save. This is made clear when van Fraassen says that the empirical success of physical theories consists 'in their success of fitting the data, the deliverances of experimental and observational experience' (van Fraassen 2006, 303). This leaves out novel predictions, an important feature of physical theories. Also, according to van Fraassen, in theoretical change there is only continuity in the most minimal sense of duplication of past empirical knowledge, i.e. intertheoretical relations have to be seen just in terms of duplicating and superseding past empirical knowledge (van Fraassen 2006 298-301), and this is a doubtful view on intertheoretical relations in physics. This makes it impossible to make sense of novel predictions in terms of the relation of general derivations to a particular benchmark derivation. That is, constructive empiricism cannot make sense of novel predictions even in a limited relative way. Thus, I consider doubtful that constructive empiricism might come out with a philosophical account of the *de facto* situation that physical theories actually predict novel unexpected phenomena in nature.

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